

# THE AGE OF THE EARTH FROM THE GEOLOGICAL VIEWPOINT.

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## GENERAL INTRODUCTION.

In pioneer days, when the sciences were struggling for a place in the sun, it fell to Geology to pull up and set back the stakes that man had stuck to mark the beginning of the earth. This seemed to many a moving of sacred landmarks; to others it seemed a wanton use of the secrets of the cemetery of nature's dead. A bitter war arose: racial bias disputing the rock beds; tradition and sentiment fighting mud layers and fossil imprints. The struggle that followed was long. The throwing of rocks and rock-ribbed arguments grew to be an art that might well have drawn forth the envy of an Ajax. But the substantial slowly gained on the sentimental. The brutal cogency of a slab of fossils could be hated and fought, but could not be gainsaid. And as the tide turned, the geologist began to play crusader; he mounted his war horse and went forth to convert the world—including, withal, some of his scientific colleagues.

After a time, however, the battle shifted to another field. Darwin and Wallace drew off a following and taught them to use the subtler weapons of "the struggle for existence" and "natural selection." However, they still plied the old geologic weapons, for they, too, had reason to point to bed on bed; they had need of even more time than the geologists. So they took the lead and the team became a tandem, Biology prancing in front, Geology trotting on in the thills.

But the spirit and abandon of this team soon awakened a new antagonist. Kelvin took the field in the name of Physics, Astronomy and Mathematics, and sought to set metes and bounds to the backward extension of terrestrial time. He told the tandem, with much show of premises and figures, that the feed on hand positively would

not let them go as far as they proposed. The tandem was reined in and marked time, losing not a little of the free natural pace it should have retained.

But in time this great antagonist was neatly flanked from an unexpected quarter. Certain physicists and chemists discovered that they had a decaying atom on hand. They keenly watched its rate of decay and soon came to see that if atoms take as long to grow as some of them take to die off, there should have been time enough for this little ball of atoms to get together—and plenty of energy as well.

So, too, astronomers began to see that the making of globular clusters and stellar galaxies required time. If 60,000 suns have time to come together and work themselves into a steady state while yet they are suns, the getting together of our little earth may be merely a negligible matter after all. And so a new order of things has arisen. The tandem is a vexed tandem no longer. We now have a fine four-horse team; Astronomy and Physics at the front, leading off at a great pace; Biology on the pole, steadying the team, and Geology plodding on as the old original wheel horse.

#### THE GEOLOGIC PROBLEM.

Now, I must hasten to warn you not to expect much of the old wheel horse. He has grown stiff in his paces, and his paces are not what they should have been. Kelvin checked him too high. A reasonable check should have given him good form and some sense of restraint, but checked too high, he took to short mincing steps. As a result he's in poor shape to swing into the great pace of the new leaders. It is too much to expect him to recover his natural step at once, but he will in time. For the present, he will need a touch of the whip now and then to make him keep pace. Let this be gentle and considerate, because of his age and his past service, but let it be persuasive.

#### REPRESENTATIVE GEOLOGIC TIME-ESTIMATES.

It is a simple matter, theoretically, to use the rate at which sediments are being laid down, or solutions gathered into the ocean, as a divisor to find the time required to lay down the whole column of sediments or the whole accumulation of the salts in the sea. Practi-

cally there are serious difficulties. In the first dozen years of this century four notable estimates were made in this way by able geologists: two American, Clarke<sup>1</sup> and Becker;<sup>2</sup> two British, Joly<sup>3</sup> and Sollas.<sup>4</sup> These estimates form an admirable point of departure for this discussion. They represent the mode of geologic interpretation that has been most current until recently; they typify opinions widely held by the conservative school of geologists; they stand out in contrast to the views of the new school.<sup>5</sup> The mean of the four estimates, on the basis of the sediments, was 90,000,000 years, roundly; on the basis of the ocean, 95,000,000 years. The highest individual estimate was 150,000,000 years; the lowest 70,000,000 years. I shall not deal with the individual estimates, but merely with their mean value, and with that only as representative.

My discussion can not be specific and concrete without some reference to views in other fields. My colleagues in this Symposium will give you the last word from their viewpoints, and if I could follow them I would gladly take their estimates as specifically representative in their several lines. In lieu of this, I can only use such general views as are current. It has long been known to be the view of many biologists that the evolution of life required much more than 100,000,000 years. It is also well known that most estimates based on

<sup>1</sup> F. W. Clarke, "A Preliminary Study of Chemical Denudation," *Smiths. Misc. Coll.*, LVI., No. 5 (1910); "The Data of Geochemistry."

<sup>2</sup> George F. Becker, "The Age of the Earth," *Smiths. Misc. Coll.*, LVI., No. 6 (1910).

<sup>3</sup> J. Joly, "An Estimate of the Geological Age of the Earth," *Trans. Roy. Soc. Dublin*, VII. (1899), pp. 23-66; "The Age of the Earth," *Phil. Mag.*, 6th ser., Vol. XXII. (1911), pp. 359-80.

<sup>4</sup> W. J. Sollas, "Presidential Address," *Quart. Jour. Geol. Soc.*, Vol. 65 (May, 1909), *Proc. Geol. Soc. of London*, Sess. 1908-9, pp. 1-cxxii.

<sup>5</sup> It is not practicable to summarize the time-estimates of the newer school consistently with the division of labor adopted in this Symposium since they are composite, embracing organic, astronomic, and radioactive factors, with some emphasis on the last. The following papers of this class may be taken as representative: J. Joly, "Radioactivity and Geology," Van Nostrand, New York (1909), pp. 233-51; Arthur Holmes, "The Age of the Earth," Harper Bros., London and New York (1913), pp. 1-196; J. Barrell, "Rhythm and the Measurement of Geological Time," *Bull. Geol. Soc. Am.*, Vol. 28 (1917), pp. 745-904; T. C. Chamberlin, "Diastrophism and the Formative Processes," XIII., "The Time over which the Ingathering of Planetesimals was Spread," *Jour. Geol.*, Vol. XXVIII. (1920), pp. 675-81.

radio-activity greatly exceed this. Astronomical opinion has recently been trending toward the view that long periods are necessary for certain typical phases of celestial evolution. Perhaps I may overstep my proper limits far enough to say that I have recently tried to form some notion of the time required for the gathering of planetesimals from what seemed a probable distribution into the collecting planetary nuclei, and found a period of the order of two or three billion years the most probable.<sup>6</sup> These current views in the collateral fields warrant me in assuming that there is a wide discrepancy between the geological estimates just cited and the present estimates in the related fields. In view of this I can perhaps serve you best by inquiring whether the recent additions to geologic evidence and the newer modes of interpretation mitigate this discrepancy in any appreciable measure. Let us consider first what the newer evidence relative to the sediments has to say, and turn later to the solutions.

#### THE TESTIMONY OF THE SEDIMENTS.

In considering possible modifications of the foregoing estimates five questions arise: (1) How far do recent investigations tend to lengthen or to shorten the older estimates? (2) To what extent has human action made the present rate of wash and deposit faster than the mean pre-human rate? (3) How far does the present state of elevation make the present rate faster or slower than the mean rate of the past? (4) How does the present area of erosion compare with the mean area? And, finally, (5) does the lower end of the geologic column give us the point from which the accumulation of the sediments began? I can try to answer these questions only very briefly and inadequately.

1. *The Effect of Intensive Studies on Earlier Time Estimates.*—A strictly accurate chronology reaching back from the present for several thousands of years is now being worked out by De Geer.<sup>7</sup>

<sup>6</sup> Diatrophism and the Formative Processes, XIII., The Time over which the Ingathering of the Planetesimals was Spread, T. C. Chamberlin, *Jour. Geol.*, Vol. XXVIII. (1920), pp. 675-81.

<sup>7</sup> Gerald De Geer, "A Geochronology of the Last 12,000 Years," *Proc. Int. Congr.*, Stockholm, 1910, p. 241; "Kontinentale Niveauveränderungen im Norden Europas," *ibid.*, p. 849; Spitzbergen, *ibid.*, p. 1205; "Phenomenes Quaternaires de Stockholm," *ibid.*, p. 1290; "Quaternary Phenomena in the Southern Part of Sweden," *ibid.*, p. 1339.



He has succeeded in identifying the yearly deposits of glacial waters and in correlating them with annual moraines. In addition to this, he has been able to match sections at distant points by comparing the succession of peculiarities in the annual "varve" layers. While this is a quite special method and has only limited application, it is important to general time estimates, because it gives a means of checking up other criteria that indicate glacial time, and these help check up certain non-glacial criteria. As is well known, the duration of the recent Ice Age was for a time a sharply battled question, and the old views pitted against the new views came to be well defined. Though not yet ready for precise announcement, it is already foreshadowed that the De Geer method of measurement, when it shall have fully covered the retreatal stage of the last glacial epoch, will show that stage to be about three times as long as it was made by the most representative of the old estimates. The main differences of opinion as to the duration of the glacial period, however, grew out of the evidence that instead of one simple short epoch there were several epochs of glaciation separated by rather long interglacial intervals. Now, by using the De Geer method to correct the criteria on which time estimates of these glacial and interglacial epochs have been based, a glacial period at least twenty times as long as that assigned by the old estimate seems to be foreshadowed. Very likely this degree of extension of an old-time estimate by a new one is exceptional; at any rate, the glacial formations are exceptional deposits and make up only a small part of the geologic column.

In considering the standard water-lain sediments of the column, it is to be understood that only rapid surveys or mere reconnaissances have as yet been made of the larger part of the earth, and that inevitably inconspicuous breaks in the continuity of the deposits have been overlooked. As a result recent critical studies have revealed in some cases surprising numbers of gaps in the continuity of deposition. For example, Dr. Stuart Weller, in a study of what was formerly regarded as a continuous section of the Mississippian, has found no less than 12 breaks in continuity. The time-value of these, in his judgment, is two or three times as great as that of the visible beds themselves.<sup>8</sup> The time-values of such intervals are best judged by

<sup>8</sup> Personal communication.

comparing the faunas below them with those above, but this falls within the province of my paleontological colleague, and I therefore leave this source of correction in his hands, merely expressing the conviction that these breaks in the continuity of the sediments are quite sure, when finally and fully adjudicated, to extend greatly the old estimates of the time occupied in sedimentation.

2. *Human Acceleration of the Rate of Deposition.*—To pioneers who watched the effects of floods, freshets and ordinary wash on the native surface of our prairies and forests in their virgin state, and who are able now to compare this with the present wash of the same surfaces under cultivation, there is no need to argue that human intervention has greatly hastened denudation and deposition. In the native state the surface was protected by thick mats of grass, leaves and other vegetal debris; while the soil was bound together by dense entanglements of roots. The waters then ran almost clear where now they run mud.<sup>9</sup> Added to this are the quickened deflation of winds, the wear of roadways, the effects of quarrying and other excavation, as well as the actual carting away of clays, sands, gravels, quarry stone, foodstuffs, timber, and other material. While it is not easy to fix on a definite measure of these effects, the needed correction seems certainly to be large.

3. *Correction for the Present Elevation of the Surface.*—It is held by leading American geologists that the general elevatory movements of the continents have alternated with periods of relative stability during which the processes of base-leveling and sea-transgression have cut down the continents and developed peneplains.<sup>10</sup>

<sup>9</sup> A fuller statement of this with citations of data from Dole and Stabler and from F. W. Clarke is given in "Diatrophism and the Formative Processes, VIII., The Quantitative Element in Continental Growth," T. C. Chamberlin, *Jour. Geol.*, Vol. XXII. (1913), pp. 522-28.

<sup>10</sup> The following group of papers emphasize the rhythmical nature of elevation and stability and of the action of the atmosphere and hydrosphere upon the periodic deformations of the earth body and thus form the basis for the arguments in this and the next section: "A Group of Hypotheses Bearing on Climatic Changes," T. C. Chamberlin, *Jour. Geol.*, Vol. V. (1897), pp. 681-83; "The Ultimate Basis for Time Divisions and the Classification of Geologic History," *ibid.*, Vol. VI. (1898), pp. 449-63; "A Systematic Source of Provincial Faunas," *ibid.*, Vol. VI. (1898), pp. 597-609; "The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmos-

The periods occupied in the process of lowering the surface by denudation are held with good reason to be greater than those occupied in its elevation by deformative action. It is needless to say that elevation increases the velocity of the run-off, and that this velocity greatly increases the transporting power.<sup>11</sup> It is generally agreed that the present altitude of the continents is greater than their mean elevation during geologic history. Geologists recognize at least two stages in which the continents were exceptionally high and broad: that which attended the transition from the Paleozoic to the Mesozoic Era, and that which attended the transition of the Tertiary to the present epoch. The existing stage thus falls in one of the most notable stages when continental elevation and breadth were greatest, though perhaps not at its climax. Geikie estimates the present mean elevation of the land at 2,441 feet.<sup>12</sup> The mean elevation of the great peneplains is a matter of judgment rather than of knowledge, but no one would probably put the elevation at much more than a third of this. Probably a third is too high. The mean elevation for all the ages, high and low, quite surely falls somewhere between 2,400 and 800 feet, and probably nearer the 800 than the 2,400. There can be little doubt, then, that the present rate of denudation and deposition is much above the mean rate.

There are incidental conditions attending high relief which add appreciably to the immediate effects of the steep declivities to which it gives rise. Relief of the surface increases the vertical air currents and these favor precipitation; they also tend to concentrate the precipitation and give it enhanced effect. High relief often induces sharp showers and distinctly rapid run-off. The smooth surfaces of the stages of lower elevation, on the other hand, favor a more even

phere," *ibid.*, Vol. VI. (1898), pp. 609-22; and specifically as applied to the question of age; "Rhythm and the Measurement of Time," J. Barrell, *Bull. Geol. Soc. Am.*, Vol. 28 (1917), pp. 745-904. The argument used in the present paper will be found stated as a quotation from Chamberlin in Holmes' "The Age of the Earth," cited above (1913), pp. 79-81. See also T. C. Chamberlin, "Diastrophism as the Ultimate Basis of Correlation," in "Outlines of Geologic History," compiled by B. Willis and R. D. Salisbury (1910), The University of Chicago Press, pp. 298-306.

<sup>11</sup> See the special investigation of G. K. Gilbert, "The Transportation of Debris by Running Water," Prof. Paper 86, U. S. Geol. Surv. (1914).

<sup>12</sup> "Textbook of Geology," 4th ed. Vol. I. (1903), p. 49.

distribution of rain, a larger absorption into the soil, and a slower run-off of the remainder. So, too, accidented surfaces are likely to be ineffectively protected by vegetation, for lack of soil, or of adequate moisture. These and other incidental influences add appreciably to the total effect. It seems clear, therefore, that a large correction is to be made to the present rate of denudation because of the relatively high elevation of the continents.

4. *Correction for Area.*—This is to a large degree, but not wholly, an effect of the elevation of the continents, but none the less it deserves separate recognition. When elevation increases the land area, base-leveling and sea-transgression at once set in and combine their forces to reduce the exposed area. The result is very large variations in the areas of the ancient lands. The estimates of Schuchert and others for North America show variations that range from the full surface of the continental platform down to half that surface. As a rule, of course, the lesser surfaces were also low surfaces, and the two influences were cumulative. At stages of low elevation and slack drainage deep soils were likely to accumulate and these favored thick vegetation which helped to hold the soils. Thus in several ways small area and low elevation united their influence in a cumulative effect which could not have been other than large.

*Partial Summary.*—Summarizing at this point, it appears that four important corrections quite certainly must be applied to the present rate of geologic action to reduce it to a mean rate for the whole of geologic time. These corrections are cumulative. There seems to be no way at present to evaluate them rigorously or perhaps even very closely. The weighing of their value is greatly affected by the individual judgment, and that in turn by individual experiences and opportunities of observation. Speaking for myself alone, it does not seem to be overstraining the importance of these corrections to suppose that their cumulative value will be found great enough to bring the old-time estimates up to figures of about the same order as those of the current radio-active estimates.

5. *The Lower End of the Geologic Column.*—Below the base of the Paleozoic series the geologic terranes are much obscured by diastrophism and metamorphism. It remains to inquire what is the



testimony of this obscured portion as to the horizon at which the sediments began to be deposited, for that is essential to measuring the whole period of deposition. It was once thought that the Cambrian beds lay close upon "the original crust," and that they either represented the real beginning of the sedimentary series or else an early stage close to the beginning. But as field work progressed it was found that first one and then another thick series of sediments lay below the Cambrian. It was further found that there were marked unconformities between these great terranes, and that these were of such a nature as to imply long intervals of time unrepresented by deposits; that is, times when the deposition took place elsewhere. The number of such strongly unconformable terranes has been notably increasing as investigation proceeds. The correlation of these is not yet complete or even wholly satisfactory, so far as it has gone, but the leading workers in this field recognize six, eight, or more great stages. This Precambrian factor is thus certainly great, but just how great is yet undetermined.

The mere extension of the sediments downwards in this large degree is not, however, the most significant feature of this recent work. Great granitic series form a prominent feature of these lower terranes. These were formerly taken to be parts of "the original crust." They have been found, however, to consist of remarkable *intrusions* into earlier series made up of sediments, volcanic debris, and surface lava flows. The granites are not evidence of "the original crust of the molten globe." Nor does there seem to be any other trustworthy evidence of "an original crust." Thus observational evidence does not give the depth at which the bottom of the column of sediments is to be found, and theory is perhaps as favorable to a depth of a thousand or two thousand miles as any shallower depth. A reliable starting point for reckoning the total thickness of the sediments is not available.

#### THE TESTIMONY OF THE SOLUTIONS.<sup>13</sup>

In the effort to find the earth's age by means of sediments advantage may be taken of the fact that each deposit makes its own indi-

<sup>13</sup> Only a brief general statement could be made at the Symposium for lack of time. Adequate citation of evidence or of authority, or elaboration of

vidual contribution. It is thus possible to sum up as many of these separate contributions as can be measured satisfactorily and rest the case there, leaving what remains of the earth's age to be found out later or to be guessed at or to be ignored. But when the inquiry turns to the solutions it must face the fact that the contributions of each stage have been mingled with those of all other stages and the record to be measured is thus an indivisible unit. If the ocean, considered as such record, can be used to measure age at all, it is the total age of the ocean. This total age of the ocean can not be expected to tally with the age found from an unknown fraction of sediments.

*The Basis of Estimating the Age of the Ocean.*—The interpretation of the time occupied in the concentration of solutions in the ocean hangs on the assumptions made relative to its origin and to the entire history of the earth's waters on land and sea alike. This includes the volume of the waters at the start and all along; it includes the metamorphic solutions from within the earth as well as those that arise from surface action. Account must also be taken of such reversals of action as take material out of solution and return it to the solid state. All these must be considered, for they are all necessarily involved in the question of the age of the ocean. Some basal assumptions are unavoidable, and if we must deal with them, it is best to be frank and explicit about them. The necessary assumptions as to the early stages of the ocean are more or less speculative, but if we are to discuss the question of age at all, there is no occasion to be squeamish about that. It does not make the assumptions any less "speculative" to gloss over or shy at the fact that they are speculative or at least have speculative factors. Assumptions are least dangerous when explicitly recognized. They are even likely to be least speculative when the grounds on which they rest are carefully sifted, logically weighed, and made to throw such light as they may on the question. The printed text gives somewhat more liberty and I have taken advantage of it to a limited extent and have recast this part of the text to accommodate it to this. I am greatly indebted to Dr. T. Wayland Vaughan, U. S. Geologist in Charge of Investigations on Sediments, for aid in securing documents and personal statements from the departments of our general government and from its officials engaged in investigations bearing on the question in hand.

tion in hand. We have no call to discuss the age of the ocean at all unless we are ready to be frank about the other end of its history. The crux of the issue lies there. We are all agreed about the age of this end.

*The Two Types of Assumptions in Actual Use and their Radical Differences.*<sup>14</sup>—Only two general types of assumptions require recog-

<sup>14</sup> The four estimates of the age of the ocean which were cited earlier and which give an average age of 95,000,000 years, with a range from 70,000,000 to 150,000,000 years, seem clearly to have been made on the basis of the inherited view of the origin of the earth. This assumes that all the material of the present hydrosphere, together with such substances of the present earth body as would be volatile at the temperature of molten rock, were held in the atmosphere which surrounded the supposed molten earth. The oceanic history is assumed to have begun when the waters from this primitive ocean-bearing atmosphere condensed upon the crust that had formed over the molten earth. The great influence which this view has had on geologic thought and the wide extent to which interpretations derived from it enter into various geologic concepts *not recognized as its offspring*, are chiefly due to the explicit teachings of the old masters who had clear cosmological conceptions and the courage of their convictions. Foremost of these among Americans was Dana, and as I once believed and taught this view but have become an apostate from it and the protagonist of another view, I trust that in following Dana's statment in the Fourth Edition of his *Manual of Geology* as a standard exposition I shall not be doing any injustice to the inherited view.

On the other hand, the only accretional view that has been carried out into any measure of detail is the planetesimal hypothesis. (The most recent statement of points pertinent to this discussion may be found in a series of articles entitled "Diatrophism and the Formative Processes," I. to XV., *Jour. Geol.*, Vols. XXI. (1913) to XXIX. (1921), particularly articles X. to XV.). To clear the air of needless fog let it be noted that this is not a speculation regarding the origin of the universe, or of the stars, or even of our sun. It is merely an endeavor to explain the singular dynamic properties of the earth and its fellow planets and their strange relations to the sun. It is merely a definite endeavor to solve a very definite problem. It started from an attempt to test the tenability of the inherited view of the atmosphere just outlined. The hypothesis that the atmosphere once held as vapor all the water of the ocean and much other volatile material, was framed before the nature of gases was known. The view seemed logical enough under the old notion of gases. Special reasons for testing it by the kinetic theory of gases arose out of the relations of the atmosphere to glaciation. The results of the test were very unfavorable. It seemed wholly improbable under the kinetic constitution of gases that a molten earth could hold so vast and active an atmosphere. This adverse result led to other tests of a more mechanical sort. These disclosed certain critical facts in the dynamics of the solar system which, while not al-

nition here, the one based on the view that the earth started as a molten globe, the other that it grew up slowly by the accession of solid particles. For the purposes of the present question it is not radically material how the molten globe arose, on the one hand, nor by what celestial mechanism the accretion took place, on the other, beyond the fact that the material of the ocean was supposed to be assembled as a vapor about a hot earth, in the one case, ready to begin work in full volume when cooling took place, while in the other case the waters came into action very gradually. Out of these basal differences, however, there arise some important contrasts in the modes of later action that are almost equally radical in their bearings on the evolution of the ocean, so that both the original and derivative differences need to be sharply in mind in considering the question of the earth's age.

1. On the one hand, it is assumed that the ocean was essentially uniform in volume throughout all the ages, and that the disintegration of the surface rocks, the inflow of solutions, and the content they carried were also essentially uniform. If these assumptions are correct, or if they hold true of a single leading element, as sodium, the present rate and content of inflow may be used as a divisor to ascertain the total time of inflow. This, however, is subject to the condition that there is no important reversal of action, or at least none that can not be adequately measured and discounted.

The alternative view assumes that the ocean grew to its present together unknown, had not been adequately recognized as indispensable criteria in the interpretation of our planetary system. In other words, the earth and its family have dynamic peculiarities that make the question of their origin a special one. *These hereditary peculiarities point the way to their interpretation.* The planetesimal hypothesis is simply the result of an attempt to follow these hereditary traits back to their parentage. It is as little as possible speculative, for it starts with mechanical properties which are rigorously determinate and which must be met by any hypothesis of genesis worthy of serious consideration. It follows these back to their probable origin in other known properties and natural actions so related to them as to be their probable sources. The method followed was only a phase of the standard practice of geologists in following the vestiges of a recorded event back to their most probable sources. If peculiar at all, it is merely in that the vestiges are dynamic. It ill becomes us to be squeamish about historical deductions from historical vestiges for there are plenty of people who regard geology a speculation from beginning to end and there is no present help for it.



volume very slowly from a small beginning, that the solutions came from three sources and were variable from the start, so that the whole history was very different from the preceding. The three sources of solutions were (1) the internal metamorphic action of waters entrapped in the growing accessions, (2) surficial action by the atmosphere and hydrosphere acting on the shell of the lithosphere, and (3) accessions of water-substance from the environing sphere under control of the sun—particularly accessions through the system of exchange between the ultra-atmosphere of the sun and the ultra-atmosphere of the earth.<sup>15</sup> The first source brought one type of solutions, the second another, and the third added water that was essentially fresh. Under this view it is obvious that until this complex of sources and variations has been worked out the present rate of accession has no claims to be regarded as a trustworthy divisor for ascertaining the total period of activity.

There are also two rather radically different methods of dealing with the geo-chemical evolution of the ocean. These are not necessarily connected with the preceding differences of view, but as a matter of fact they are closely associated with them.

The first of these—associated with the older cosmological view—takes for its start the concept of a universal crust acted upon from without by an atmosphere and hydrosphere, for its middle factor the streams, and for its end-products the sediments and the ocean. The matter in the sediments and in the ocean taken together are supposed to match the loss of the crust by decomposition and wear. Under this view any real failure to so match is a discrepancy to be accounted for. In the special problem in hand the sodium in the ocean, together with the sodium that remains in the sediments, should match the sodium once in the denuded rocks of the crust. So, also, the other elements of the crust should appear in due proportion in the sediments and the ocean. It is recognized in the cases of calcium, magnesium, potassium, silica, and other elements that there are reversals of action by which these elements go back into the solid state as new sediments, but it is held that sodium does not return to the solid state in the sediments in a similar chemico-physical cyclic way, to any appreciable

<sup>15</sup> "The Origin of the Earth," The University of Chicago Press (1916), pp. 19-21.

degree. Thus the sodium now in the ocean is held to represent the accumulation of all the geologic ages, and this total accumulation divided by the rate at which the present streams are carrying sodium down to the sea is held to give the age of the ocean, barring some corrections to be noted later. The crux of the whole issue of age lies in the validity of these concepts, particularly the irreversibility of the sodium.

The other view is far less simple. It looks upon the hydrosphere, of which the ocean is the chief concentration, as only the liquid phase of a solid-liquid-gaseous cycle through which the earth substances are passing. It is held that the earth is perpetually undergoing self-metamorphism in all its parts. This metamorphism takes place in a multitude of ways, each unit doing its part, in its own place, in its own way, and at its own rate. Each unit passes through its own cycles of liquid-solid-gaseous states according as its nature, its contacts, and conditions determine. Its career is wholly dependent on its own succession of conditions, and is only affected by what other units are doing under their conditions incidentally as it happens to come into working relations to them. The cycles that thus arise are so multitudinous and intricate that their correlation is a most formidable task which is scarcely yet fully appreciated; little more than a beginning has been made toward its accomplishment.

Under this view it is necessary to stress the fact that the simple solid-to-solution change from the rock to the ocean does not cover the whole evolution in the case of any substance. In most cases there are many cycles, some in parallel lines, some in succession. The content of the indurated rocks, on the one hand, and the content of the sediments and the ocean solutions on the other, are great features that have guiding value, but they are too general to cover with adequate accuracy the sub-cycles that make up the real history. The correlation of the whole is too largely conditioned by the number and speed of the constituent cycles to be successfully dealt with. It is especially affected by the reversals from the liquid to the solid state which take place during the passages from rock to soil, from soil to fresh-water solutions, or to colloids and turbid suspensions, and from these to the concentrated sea solutions in the borders of the sea. The

deductions drawn from such a complication of cycles differ in very important respects from the deductions drawn from a simple matching of the content of the igneous rocks with the content of the ocean solutions and the sediments.

Now, in respect to sodium, it is, of course, recognized that it returns to the solid state in less degree than potassium, magnesium, and calcium. It is held, nevertheless, on good evidence, that the sodium does return to the solid state in minor equilibrium degree and is recounted. The reactions involved are controlled by the law of mass action and the mutual effects of the constituents on one another. The reactions are particularly affected by the degrees of concentration, which are very low in the fresh-water solutions and quite high in the sea solutions. The trend of the reactions is toward equilibrium between the constituents, not toward any exclusive or monopolistic combination. Specifically, it is held that when the state of concentration favors the sodium, it will displace either potassium, magnesium, or calcium, and that such displacements take place as a standard feature in the processes of disintegration and solution, though only in an appropriate minor degree.

Let us now turn to such determinations as are available for testing the validity of these contrasted interpretations.

*Discrepancies in the Matching of Igneous Rocks with Sediments and Solutions.*—The differences between the content of the igneous rocks and that of the sediments and the salts of the ocean have been put in definite form by Leith and Mead.<sup>16</sup> Comparing first the igneous rocks with the sediments, they find the following excesses and deficiencies: (1) a *deficiency* of 3.1 per cent. in iron; (2) a *deficiency* of 26 per cent. in magnesium; (3) an *excess* of 32 per cent. in calcium; (4) a *deficiency* of 64 per cent. in sodium; and (5) an *excess* of 2 per cent. in potassium. If the corresponding constituents in the ocean are added to these severally, some of the discrepancies will be lessened, while others will be increased; the discrepancies do not disappear, though they are somewhat mitigated.

2. It is recognized on all hands that the land waters vary greatly according to the nature of the drainage area from which they are

<sup>16</sup> C. K. Leith and W. J. Mead, "Metamorphic Geology" (1915), p. 69 *et seq.*, particularly pp. 83-88.

derived. In some districts they consist largely of carbonates, or of sulphates; in others of chlorides, or of silicates; while the degree of dominance varies greatly within each class. The solutions of the ocean, however, are not identical with any of these, nor with a simple mixture of them; the ocean solutions are dominantly chlorides, but constitute a combination which is quite distinctive. This implies that, instead of a theoretical mixture of the land waters, an effective chemico-physical reorganization takes place, a liquid metamorphism of the heterogeneous land waters and their content into the homogeneous sea solution and its sediments. This is in a measure recognized, but the recognition is inadequate if the change is regarded simply as a liquid metamorphism. There is a neglected solid factor in the form of silts and clays that is of critical importance. The usual comparison is really between the *clear* waters of the streams—which are mainly the outflowing *ground* waters of the land—and the sea waters. The *run-off* and its contents—the wash-waters of the land and their burden of mud—are neglected. But it is this run-off water with its mud and the colloids that go with it which carry the larger part of the acid radicals of the soil from which the basic radicals were leached. The reunion of these acids with the alkalies in the border of the ocean constitutes a critical part of the metamorphism which gives rise to the ocean solutions and sediments. We will return to this presently.

3. *The Larger Part of the Solutions now Flowing into the Ocean Comes from the Sediments; the Lesser Part from the Igneous Rocks.*—This becomes the more suggestive when it is noted that the sediments have been worked over repeatedly in some notable part; some small part, perhaps hundreds of times; some larger part, scores of times; while some other large part perhaps has not been worked more than once, unless we count in the many times most material is handled in going from the parent rock to the ocean. That the sediments should still be able to yield saline solutions to the observed extent raises a vital question into which it is necessary to inquire before assuming the practical non-reversibility of the sodium solutions. It is already well recognized that a part of this sustained productiveness is due to sea-winds which carry salt inland from the



ocean. This salt is thus counted as many times as it is carried back. An endeavor has been made to estimate and make allowance for this by taking the increase of salt solutions near the ocean as a criterion. It has also been recognized that salt solutions are entrapped in the pores of the sediments as they are laid down under the sea, and that when the beds are afterwards raised above the sea-level these solutions are drained into the streams and counted again as salts from the land. The amount of duplication involved in this depends on the ability of the rocks to hold salt water mechanically in their pores, and correction has been sought by estimating their porosity and discounting for it. Sandstones usually have the highest porosity and limestones come next, while shales are relatively close-textured and impervious, but still the shales are exceptionally productive. So, also, it has been recognized that beds of rock-salt occur in the stratified series, but these are held to be relatively unimportant. So still further some particles of the original rock may remain undisintegrated; so, too, fresh particles may be cut away from exposed rocks by wind blast and widely though sparsely distributed. But when the modifying effects of all these have been recognized and discounted, there still remains a serious source of double counting of sodium which we must consider presently.

4. The ratio of chlorine to sodium is a crucial matter, recognized but not sufficiently emphasized. Inspection of the drainage from regions of igneous rocks shows that *the chlorine is relatively low and the sodium relatively high* compared with the ratio of these elements in the ocean, which is about 1.8 chlorine to 1 sodium. The relative deficiency of chlorine in the drainage from the very rocks that are assumed to be the ultimate source of the salt solutions raises a fundamental issue.

5. In view of this, let us make our inspection as sweeping as possible. Let us compare the ratio of sodium to chlorine in the ocean with the ratio found in the average igneous rock of the whole "crust." The latest and most authoritative determination of the chemical composition of the igneous rocks is that of Clarke and Washington, which gives the mean sodium content as 2.83 and that of chlorine as 0.096.<sup>17</sup>

<sup>17</sup> Frank W. Clarke and Henry S. Washington, U. S. Geol. Surv. and Geophys. Lab., Carnegie Institution of Washington, "The Average Compo-

From this it appears that the mean per cent. of sodium in average igneous rock is *about thirty times as great as their content of chlorine*. This is a large difference, but it does not represent the full discrepancy, for the chlorine in the ocean exceeds the sodium in about the proportion of 1.8 to 1. Taking this into account, the discrepancy rises to somewhat above 50 to 1. This is a formidable discrepancy. How is it to be met on the assumption that the sodium in solution is not reconverted into sodium solids, but remains in perpetual solution? The dilemma is not much relieved by reckoning in the sediments and the ocean salts, for Clarke and Washington also give<sup>18</sup> the ratio of sodium to chlorine when the atmosphere and hydrosphere are reckoned in with the outer ten miles of the lithosphere. The discrepancy, corrected for actual oceanic proportions, is even then nearly 20 to 1. Quite naturally volcanoes have been thought to be a source of excess of chlorine, but any contribution from the volcanoes is covered by this inclusion of the whole atmosphere and hydrosphere in the average. Besides, the later studies of volcanic gases do not sustain the earlier views that they contained a specially high content of chlorine.<sup>19</sup> The observed differences between the sodium and the chlorine appear to have grown mainly out of the normal processes of cyclic change when these are viewed in their largest aspects. If the sodium returns to the solid state in due (though lesser) proportion to the potassium, magnesium, calcium, and chlorine, as these constituents are found mixed in the solutes and the sediments, there is no necessary discrepancy in these great differences. The discrepancy is constructive and is imposed by the assumption that the sodium does not take its proportional part in cyclic action. Under the alternative interpretation, the amounts of the several elements present in the ocean are primarily functions of their own cyclic histories; their proportions

sition of Igneous Rocks," *Proc. Nat. Acad. of Sci.*, Vol. 8, No. 3 (May, 1922), pp. 108-13. In the paper as read at the Symposium I used the then latest and most authoritative figures, viz.: those of H. S. Washington, "The Chemistry of the Earth's Crust," *Jour. Franklin Inst.*, in which the sodium was given as 2.85 and the chlorine as 0.055.

<sup>18</sup> *Ibid.*, p. 114.

<sup>19</sup> E. T. Allen, "Chemical Aspects of Vulcanism with a Collection of the Analyses of Volcanic Gases." Papers from the Geophysical Laboratory, Carnegie Institution of Washington, No. 440 (1922), pp. 1-80.

are not predetermined solely by the composition of the igneous rocks now at the surface, but rather by the relations of their own solvent to their non-solvent natures under the conditions of their long complex history. Specifically, in the case of sodium and chlorine, the observed ratio merely means that the solution stages of sodium compared with its solid stages are much inferior to those of chlorine, just as those of potassium are much inferior to those of sodium, and so on through the list. But, however cogent this may be, definite evidence that sodium does enter freely into the cyclic processes, in due proportion to the action of its associates, however inferior the proportion may be, will naturally be demanded. Allusion has already been made to a neglected factor. Let us turn to that.

*The Mud Cycle Actuated by the Surface Floods.*—Familiar as this is in many respects, it has perhaps received less critical geochemical study than almost any other common feature of nature with which we are directly, not to say unpleasantly, brought into contact. The agricultural chemists have naturally been preoccupied with those elements of the soils that serve as plant food, the students of hygiene and domestic science, with waters suitable for drinking and culinary purposes, and the geologic chemists with the organic extracts and precipitates that form the limestone, dolomites, and siliceous beds. The mud factor of the surface wash has been neglected. And yet the muds (later shales) comprise much the largest part of the solid residue of disintegration. This solid disaggregated residue and the colloids associated with it are separated from the true solutions in large measure at the very start on their long journey to the sea. The true solutions are largely formed by waters that descend through the soils into the underlying formations and thus form the ground waters which pass by springs and seeps into the streams, giving them their steady supply of clear water. This is the water chiefly analyzed and taken into account in reckoning the material borne by the stream to the ocean. The solid residue, the clays, silts, and sands, however, are only slightly removed by the gentler rains which soak into the ground. They are carried down to sea chiefly by the floods following heavy storms, or by the thaw-waters of winter snows which form the spring freshets, or by flood stages from any cause. The turbid mat-

ter of these muddy waters contains a large part of the acid radicals with which the basic radicals of the true solutions were united in the parent rock and in the soils. While it is known that the muddy waters contain hydrous silicates of alumina and iron, partly colloidal and partly non-colloidal, together with finely divided siliceous silts and colloidal silica, full and exact information is lacking. Dr. Collins says that "the dissemination of silica in natural waters, particularly turbid waters, is one of the least accurately known of the determinations of substances present in appreciable quantities."<sup>20</sup> He adds that even "the exact state of the silica present in a perfectly clear water is usually not known. It may be colloidal or it may be present as a silicate radical." In addition to this—or perhaps the cause of it—investigation is embarrassed at the inland end of the cycle by the fleeting and irregular nature of the freshet stage, and by the rapid and intricate changes within the soils. The changes in the soil are so rapid in certain respects that F. H. King found it important to make his determinations of water-soluble solutions by means of an improvised laboratory in the field so that determinations might be made as promptly as possible after the sample tube had taken the soil from its natural relations.<sup>21</sup> At the sea end of the cycle the recombinations of the acid radicals with the basic radicals seems to take place chiefly at the base of the turbid water as it is carried out over the concentrated sea solutions and diffuses into them. Before the acid radicals reach the bottom the reversing phase of the cycle has probably ended and a new cycle begun under sub-oceanic conditions. The experimental evidence in support of this conclusion is buried in a great mass of literature which relates primarily to other elements, particularly the elements that form plant foods, such as potassium, phosphorus, etc., and those that form precipitates such as calcium and magnesium carbonates, but when these scattered data are gathered together their combined import is sufficient to make clear the essentials of what happens.<sup>22</sup>

<sup>20</sup> W. E. Collins, Chief of the Quality of Water Division, U. S. Geol. Surv. Personal communication.

<sup>21</sup> Bull. 26, U. S. Dept. Agri. (1905), pp. 26-27.

<sup>22</sup> The following are among the more important early investigations:

Way, *Jour. Roy. Agri. Soc.*, Vol. 11 (1850), pp. 313-79; Vol. 13 (1852), pp. 123-43; Vol. 15 (1854), p. 491.



The present status of knowledge and opinion is summarized by the following quotations.

Dr. Truog writes:<sup>23</sup>

The minerals or salts in soils consist largely of silicates. On weathering the bases are removed from the silicates, leaving acid residues or acid silicates. These acid silicates will react with salts like KCl and NaCl and remove the base and leave HCl in solution. When soil is treated with equal molecular strengths of these two solutions, the potassium is removed to a greater extent than the sodium. This is due to the fact that the potassium forms more insoluble compounds with the acid silicates than the sodium. Furthermore, silicates which have not had their bases removed will also react with these salts and exchange bases with them. For example, potassium chloride will react with an insoluble sodium silicate in which reaction the potassium replaces the sodium and the sodium is left in solution as soluble sodium chloride. If an insoluble potassium silicate were treated with a solution of sodium chloride some of the sodium would replace the potassium and some potassium would thus go into solution as the soluble chloride. This, however, would not proceed to as great an extent as the previous reaction, since the potassium forms a more insoluble silicate than sodium. In reading some of the literature on this subject one may get the impression that sodium is not retained by soils like the potassium, but this is really not the case; the action is merely relative. The potassium is retained to a greater extent simply because it forms more insoluble compounds with the soils.

Dr. Milton Whitney<sup>24</sup> writes:

The investigations of this Bureau<sup>25</sup> show that the absorptive power of a

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Eichhorn, *Pogg.-An.*, Vol. 125 (1854), p. 126.

Voelcker, *Jour. Roy. Agri. Soc.*, 2d series, Vol. I., pp. 289-316.

Kullenberg, Hoffman's "Jahres bericht der Agrikultur Chemie," Vol. 8 (1865), p. 15.

Lemberg, *Seitschr. deutsch. Geol. Gesell.*, Vol. 29 (1877), p. 483.

<sup>23</sup> E. Truog, Soil Chemist, Dept. of Soils of the College of Agriculture, University of Wisconsin. Personal communication.

<sup>24</sup> Milton Whitney, Chief Bureau of Soils, U. S. Department of Agriculture. Personal communication.

<sup>25</sup> These include numerous publications containing many analyses as well as special discussions, but as in all agricultural publications, the constituents that most concern plant life receive most attention and data relative to sodium is incidental. The following may be cited:

Cameron, F. K., and Bell, J. M., "The Mineral Constituents of the Soil Solution," U. S. Dept. Agr., Bur. Soils, Bull. 30, 1905.

Cameron, F. K., and Patten, H. E., "The Distribution of Solute between Water and Soil," *Jour. of Phys. Chem.*, Vol. II., pp. 581-93, 1907.

Patten, H. E., "Some Surface Factors Affecting Distribution," *Trans. Amer. Electrochem. Soc.*, Vol. 10, pp. 67-74, 1906.

Patten, H. E., and Gallagher, F. E., "Absorption of Vapors and Gases by Soils," U. S. Dept. Agr., Bur. Soils, Bull. 51, 1908.

soil resides almost wholly in the ultra clay or the colloidal material in the soil. This ultra clay is mainly a hydrous silicate of alumina and iron, with hydrated oxides of iron and probably alumina and absorbed calcium, magnesium, sodium and potassium. It is of a colloidal nature, and can be separated from the soil in the form of minute droplets in dilute colloidal solutions which form into colloidal aggregates when the concentration is somewhat over one gram per 1000 c.c.

The chemical analysis of the soil colloids which we have separated shows considerable amounts of lime, potash, soda and other material which we believe to be absorbed in colloidal condition. We believe there is a distribution between the amount so absorbed and the concentration of the non-colloidal part of the solution. We believe also that the absorption of any one of these constituents such as potassium will be influenced by the presence of other salts such as sodium or calcium. Under all stable conditions there will be an equilibrium between the amount absorbed and the concentration of the surrounding liquid. Sodium chloride lowers the absorption of potassium chloride and calcium salts lower the absorption of potassium chloride. In general soils and the colloids obtained therefrom absorb the basic ions much more readily than they absorb the acid ions.

According to Clarke the earth's crust contains 3.28 per cent. of  $\text{Na}_2\text{O}$  and 2.96 per cent. of  $\text{K}_2\text{O}$ . Thirty soils and the colloids obtained from the same collected by this Bureau contained in the soil 1.59 per cent. of  $\text{K}_2\text{O}$  and 1.45 per cent. in the colloids. The soils contained on the average 0.77 per cent. of  $\text{Na}_2\text{O}$  and 0.29 per cent. in the colloids. These figures show very clearly the greater power possessed by the soil colloids to absorb and to hold back potash than they have for sodium.

A diffusion experiment with a soil colloid lasting over two months in which large volumes of distilled water were allowed to act showed a loss of 25 per cent. of total  $\text{K}_2\text{O}$  and over 95 per cent. of the total  $\text{Na}_2\text{O}$ .

There is no question that the soil colloids are able to absorb  $\text{NaCl}$ . This is shown by the ancient experiments of making sea water drinkable by filtering through soil filters.

Data and references examined show that under conditions of leaching by rain water where equilibrium conditions are changed potassium is largely retained by the soil but sodium is largely leached out.

In the presence of much  $\text{NaCl}$ , as is found in sea water, ocean shore deposits would undoubtedly absorb considerable  $\text{NaCl}$  up to the point where the colloids were in equilibrium with the sea water. If the material were then formed into a shale and elevated to land areas the induration would presumably destroy the colloidal properties leaving the  $\text{NaCl}$  free from its colloidal entanglements and with the change of the solvent from sea water to rain

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Patten, H. E., and Waggaman, W. H., "Absorption by Soils," U. S. Dept. Agr., Bur. Soils, Bull. 52, 1908.

Schreiner, Oswald, and Failyer, G. H., "The Absorption of Phosphates and Potassium by Soils," U. S. Dept. Agr., Bur. Soils, Bull. 32, 1906.

Parker, E. G., "Selective Adsorption by Soils," *Jour. of Agr. Research*, Dept. Agr., Vol. I., No. 3 (Dec. 10, 1913).

water equilibrium conditions would be expected to remove readily a considerable amount of the  $\text{NaCl}$  while the  $\text{K}_2\text{O}$  would be largely retained on the weathering of the shale and the reformation of colloids resulting therefrom.

From these authoritative statements of present knowledge and opinion; from the early experiments of Way, Eichhorn, Kullenberg, Voelcker, Lemberg, and others, in mingling soils with salt solutions and in passing salt solutions through soils, and from many intermediate experiments cited by Sullivan,<sup>26</sup> there is left little ground for doubt that when the acid radicals previously separated from the basic radicals under conditions of very low concentration, again meet basic radicals under conditions of high concentration in the ocean off the mouths of the streams, reunion takes place in equilibrium proportions. The experiments of Lemberg are particularly instructive on this point. He treated potassium-aluminum silicates with sodium-chloride solutions of different degrees of concentration, and after thoroughly washing the solid material so treated found *that potassium had been replaced by sodium* in increased quantities as the concentration of the sodium solution was increased. Complete replacement of the potassium by the sodium did not take place, but only replacement to the degree required by the law of equilibrium. Now if, in addition to laboratory results, we recall that in former times salt water was freshened for use by passing it through soil, the periodic flooding of the border waters of the ocean by soil wash from the lands may be looked upon as a natural process of salt-water freshening. As there was wash from all the lands, and as the shales formed from the wash products are known to make up much the largest part of the sediments, the process was really one of great magnitude.

As the recombinations are divided among the constituents in accordance with the law of equilibrium, the sodium gets a smaller share than the potassium, but it gets a share. From the imperfect evidence one may guess that the sodium recombines to a third or a fourth of the extent of the potassium, but whether more or less than this proportion, it seems clear that enough sodium reunites with the acid radicals in their solid state to vitiate the use of sodium solutions as a criterion of age. This is as far as the present issue requires me

<sup>26</sup> Eugene C. Sullivan, "The Interaction between Minerals and Water Solutions with Special Reference to Geologic Phenomena," Bull. No. 312, U. S. Geol. Surv. (1907), pp. 7-62.

to go. Doubtless other cycles follow in the sea and in the sedimentary beds, particularly when deformations take place or igneous and metamorphic actions follow, but we need not dwell on these.

*The Cycles of Chlorine.*—The climax of the solvent actions that enrich the sea is reached in the cycles of chlorine, but only a passing word can be given to these. The tenor of experiments with soils indicates that chlorine remains more persistently in solution than the sodium and associated substances. As the cycles of each substance spring from its own nature and the conditions it encounters, the very high preponderance of chlorine over sodium in the ocean finds its chief explanation in this more persistent solubility. Its proportion in average rock is only a conditioning factor and is not the chief controlling influence. When compared with sodium, which is much more abundant in the igneous rocks and indeed in the whole substance of the outer ten or twenty miles of the earth shell, atmosphere, and hydrosphere included,<sup>27</sup> the logical conclusion is that the cycles of chlorine have always had a much larger liquid phase than those of sodium, and that this has been cumulative through the ages. Chlorine is better fitted than sodium to be used as a criterion of age, but even in this last case there are formidable difficulties. Both sodium and chlorine and all the other constituents, as already noted, have their own histories which are difficult to disentangle. As Roger Bell neatly puts it: "There are as many histories to be written about the waters as there are kinds of sediment."<sup>28</sup> There would be an ocean highly charged with chlorides if there were no sodium in the earth at all. So there would be an ocean highly charged with sodium solutions if there were no chlorine in the earth. The status of the ocean at any time is simply the equation of the solution phases of the antecedent cycles of its constituents, all of which have passed through long, complex, more or less individual histories. In the tedious work of their disentanglement the older and simpler geo-chemical notions will not answer; the newer principles of chemistry, physics, and geology are indispensable.

*Conclusions.*—Our finding, then, in respect to the age of the earth from the geological viewpoint is this:

<sup>27</sup> Clarke and Washington, *ibid.*, p. 114.

<sup>28</sup> Personal communication.



1. Estimates of time based on the well-preserved series of geological sediments will, when adequately corrected, probably fall into harmony with the revised deductions from paleontology, radio-activity, and astronomy, so far as these cover the same ground.

2. The distorted and metamorphosed terranes below the well-preserved series of sediments do not disclose the starting point of sedimentation. The sediments can therefore give no verdict on the *total* age of the earth; they are great enough, however, to show that the earth is very old.

3. The science of hydrogeology, of which oceanology is only a part, is not yet ready to render a verdict; it has more need of a court of inquiry than a place on the witness stand.

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